Wilson Bull., 111(4), 1999, pp. 585–588

Commentary

A CRITIQUE OF WANG YONG AND FINCH'S FIELD-IDENTIFICATIONS OF WILLOW FLYCATCHER SUBSPECIES IN NEW MEXICO

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In a recent paper in the *Wilson Bulletin*, Wang Yong and Finch (1997; henceforth Y&F) reported that they subspecifically identified 83 of 84 Willow Flycatchers (*Empidonax traillii*) captured, banded, and released in central New Mexico in spring and autumn 1994 and 1995. Given the nature of these subspecies and the means by which Y&F apparently identified them, I am extremely doubtful about the reliability of their determinations and thus the validity of these as scientific data. The fact is that identifying these taxa is quite difficult, even for trained taxonomists working in the laboratory under the best protocols and conditions. This difficulty stems from a number of factors, the major one being the pervasive subtlety of the plumage-color characters by which these subspecies mainly differ. Not surprisingly, these differences are difficult to describe in words, which is exacerbated by the fact that none of the available classification systems accurately portrays the range of plumage coloration observed in this flycatcher (e.g., Browning 1993). This means that this species' plumage-color characters are best observed in specimens (i.e., study or flat skins), which also provide the best avenue for identifying subspecies. To do this, one must first assemble series of skins representing all relevant taxa, as well as such important subcategories as age classes (e.g., adult vs immature) and seasonal groupings (e.g., spring vs autumn). Then one sorts "unknowns" (which could include live birds) into subcategories and compares them to the taxa therein, which should produce at least tentative subspecific identifications. In fact, this is the standard laboratory approach for identifying color-based subspecies, and it is the only means proven reliable for this purpose in the Willow Flycatcher.

As my earlier comments suggest, I do not believe Y&F used the approach described above in their attempts to identify subspecies in the Willow Flycatcher. In other words, they did not take synoptic series of study skins into the field, against which the birds they captured were compared to determine subspecific identities. However, I cannot be 100% certain about this because the methods section in their paper is so incomplete and otherwise deficient one can only guess at many aspects of their approach. Nonetheless, it seems logical that if they had used skins as the basis for their identifications, they would have said so. Given this assumption, if they did not use skins, how did they go about identifying their birds to subspecies? On this matter Y&F are at best vague, providing a few clues but no definitive explanations of their identification methodology. For example, we are told that they ". . . adopted the four-subspecies classification system of Hubbard (1987) and Unitt (1987)," in which "subspecies identity . . . is based [in part] on . . . coloration of the head [= crown] and neck [= forenape] and its contrast with the back, and the contrast between the breastband and the throat (see Phillips 1948, Hubbard 1987, Unitt 1987, Browning 1993)." Based on this, I assume that Y&F chose literature descriptions (as opposed to specimen comparisons) as the basis for their identification of Willow Flycatcher subspecies. In addition, I also suspect they converted these descriptions into the color values of Smithe (1975), as this is the system they used to classify coloration in birds captured in the field. Beyond this, one could also speculate on such matters as (a) how converted values were actually used to identify birds, e.g., whether in a dichotomous key, probability table, or other

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framework; or (b) what Y&F's perceptions were of color characters in various races, given that no such descriptions were offered by them. However, I see no purpose in further speculation concerning these or other aspects of their methodology. This is because if they did base their identifications on the literature rather than specimens, I believe the process became so flawed that the details are irrelevant—like rearranging deck chairs on the sinking Titantic!

The message here is that the literature is no substitute for specimen comparisons for anyone attempting to identify Willow Flycatcher subspecies, at least if attaining the most reliable scientific data is the goal. Furthermore, given logistical and other problems, I doubt even specimen comparisons would consistently yield reliable identifications of live birds under field conditions. Not only would it be unwieldy to take and use museum skins in the field, but setting up and maintaining constant conditions (e.g., lighting) would also be difficult. In addition, except for recaptures, only one opportunity would be available to identify each live bird in the field. This means that one could not reassess identifications at a later time, which is both frequent and necessary when studying specimens in the laboratory. In this regard, photographs and certainly color readings (e.g., from Smithe 1975) would not be adequate for such reexaminations because these do not exactly duplicate colors observed in the birds or specimens themselves. Given these considerations, I believe that identifying subspecies in the Willow Flycatcher is best done in the laboratory, using study skins examined under proper protocols and procedures by people trained in the process. In other words, this is a task that should be left to an alpha-taxonomic approach, which is appropriate when one considers that subspecies arose and largely remain as products of that realm.

Even when approached as outlined above, the reality is that not every specimen or even population of this flycatcher can be reliably assigned to subspecies. Intergradation and overlap occur in all characters that distinguish these taxa, so birds exhibiting such characteristics may be un- or misidentified as a result. In addition, characteristics in some populations remain poorly known, mainly because of the paucity of specimens from these areas. For example, in the latest revision of the species, Browning (1993) could only assemble 270 specimens of breeding season adults—including fewer than 20 of the endangered subspecies *E. t. extimus* of the Southwest. As a consequence, it is not surprising that he questioned boundaries between four of the five subspecies recognized in his paper. Even when populational characteristics are better known, opinions may differ as regards their taxonomic treatment. Thus, Browning (1993) recognized two subspecies (i.e., *E. t. traillii* and *E. t. campestris*) as breeding in the region east of the Rocky Mountains, whereas Unitt (1987) merged the latter with the nominate form. Differences in opinion also exist on a broader scale, such as concerning the overall number of subspecies recognizable in the Willow Flycatcher. For example, some taxonomists maintain that none should be recognized (e.g., Mayr and Short 1970, Traylor 1979), while others accept four to six as valid (e.g., Phillips 1948, Aldrich 1951, Wetmore 1972, Oberholser 1974, Unitt 1987, Browning 1993). Thus, although specimen comparisons provide our only reliable means for identifying subspecies in this flycatcher, this approach must be used with the clear recognition that it is just the first step in this very difficult endeavor.

Incidentally, the above differences in taxonomic opinion present a problem for those that rely largely or entirely on the literature for their knowledge of geographic variation in this species. That is, how does one choose which authorities to follow and thus which viewpoints to accept on this subject? Among others, one way around this would be to adhere strictly to a single point of view, such as the recent revision of this flycatcher by Browning (1993). However, Y&F chose not do this, instead electing to cobble their concept of variation from a variety of sources (e.g., Phillips 1948, Hubbard 1987, Unitt 1987, Browning 1993). Given the lack of consensus among these sources, this was a questionable decision. In fact, it would be a challenge even for people with firsthand experience with geographic variation in this species, as seen from the variety of opinions cited above. As a consequence, it is not surprising that I would quibble with Y&F's choices, including that of which authorities to follow.

For example, as indicated earlier, they cited my unpublished paper (Hubbard 1987) as a basis for the "four-subspecies classification system" adopted in their study. However, that so-called system was actually a cobbling job itself, my aim being to summarize color characters of various subspecies from the treatments of Phillips (1948), Aldrich (1951), Wetmore (1972), and Oberholser (1974). As such, it was not meant either to provide definitive descriptions of these subspecies or to recommend which should be recognized as valid. For it to have been otherwise used by Y&F may seem flattering, but it certainly was not a sound decision from a taxonomic viewpoint.

Given the flawed nature of their approach, it is no surprise that Y&F's findings on Willow Flycatcher subspecies would also be open to question. For example, when compared with what is known from specimens (e.g., Hubbard 1987), significant differences emerge on the New Mexico status of three of the four taxa recognized in that study. (In light of the relative scientific standing of the two sources, I would obviously accept the specimen version over that of Y&F in every case.) The most significant difference occurs in the subspecies *E. t. brewsteri* (sensu stricto), which breeds along the Pacific slope of North America. Although occurring regularly in migration eastward to Arizona (Monson and Phillips 1981), this form has rarely been collected east and north of that state, e.g., in Utah (Behle 1985), Colorado (Bailey and Niedrach 1965), Oklahoma (Sutton 1967), and Texas (Oberholser 1967). Hard data from New Mexico clearly conform to this pattern, with only two (4.7%) of the 43 specimens so attributed in Hubbard (1987) and even these were somewhat equivocal. By contrast, Y&F identified 33 (39.8%) of their 83 birds as *E. t. brewsteri,* which is about 8.5 times more frequent than reported by Hubbard. Another notable departure involves the subspecies *E. t. traillii* (in which Y&F include *E. t. campestris*), which breeds from the Great Plains to the northeastern Atlantic Coast. In the Southwest, *E. t. traillii/campestris* occurs regularly in the plains of eastern Colorado (Bailey and Niedrach 1965) and New Mexico (Hubbard 1987), but it has not been collected as far west as Arizona (Monson and Phillips 1981). Yet Y&F reported that 8.4% of their birds were this form, even though the the middle Rio Grande Valley lies some 200 miles west of the nearest specimen localities in New Mexico. Finally is the race *E. t. adastus,* which breeds widely in the interior U.S. north of the southwestern states, through which it passes in both spring and autumn. In New Mexico, it comprised 25.6% of the specimens reported by Hubbard (1987), compared to 10.8% in Y&F's sample.

As for the fourth subspecies (*E. t. extimus*), Y&F identified 34 (41.1%) of their birds as this form, compared to the 48.8% from throughout New Mexico by Hubbard (1987). Thus, on the face of it, their findings would seem not to differ significantly from what is known from specimens of this taxon. However, the number of questionable literature records of this subspecies suggests it may be more subject to misidentification than certain other forms, such *E. t. brewsteri* and *E. t. traillii* (both sensu lato). Birds that might be mistaken for *E. t. extimus* could include sun-bleached or worn individuals of other races, as well as pale variants of *E. t. adastus,* intergrades between the latter and *E. t. extimus,* and carelessly-examined *E. t. campestris.* If so misidentified, such instances could help explain records of *E. t. extimus* from areas outside its known breeding range, such as the northern two-thirds of Colorado (Bailey and Niedrach 1965) and Texas east of the Trans-Pecos region (Oberholser 1974). As for New Mexico, I am dubious of *E. t. extimus* records from the eastern plains, such as two specimens reported in Hubbard (1987) from Roosevelt County. In addition, I have definitely reidentified two of the purported *E. t. extimus* from that report, one from San Juan County (= *E. t. adastus* > *extimus*) and another from Socorro County (= *E. t. extimus* > *adastus*). Of course, as mentioned earlier, we do not have the luxury of reexamining *E. t. extimus* (or other subspecies) reported by Y&F, so their identifications cannot be reassessed in light of potential sources of misidentification. Given this and their flawed methodology, I see no reason to regard their findings on this form as any more acceptable than those on the other races reported in their paper. As a final point, Y&F make no mention of the differences between their findings on the various subspecies and the specimen record as discussed above.

While the need for this would not have been obvious as regards *E. t. extimus* and perhaps even *E. t. adastus,* this could hardly have been the case with *E. t. traillii* and especially *E. t. brewsteri.*

To summarize, geographic variation in the Willow Flycatcher mainly involves subtle differences in plumage coloration, concerning which taxonomists disagree in terms of the number of subspecies that should be recognized. Anyone contemplating identifying these subspecies should do so with these caveats in mind, as well as by approaching the process through the use of specimen comparisons—preferably in the laboratory under controlled conditions and with proper training in alpha-taxonomic procedures. Given that Y&F's approach appears to have been otherwise, I submit that their field identification of these subspecies cannot be regarded as a bona fide assessment of this parameter in the birds they processed in New Mexico in 1994 and 1995. Furthermore, for those that would use their subspecific findings, I urge them to do so with extreme caution to say the least. Beyond this, I would like to state that as an alpha-taxonomist, I am dismayed that a study with such a flawed approach to subspecies identification could make its way into print in a major ornithological journal. To wit, ornithology has come to rely almost entirely on non-specimen data for monitoring the distribution and status of birds on this planet. While not necessarily a bad thing, sometimes we may fail to recognize the very real limitations of such data. No better example of this exists than as regards the identification of difficult taxa, of which subspecies in *Empidonax traillii* provide a perfect case in point.

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Wilson Bull., 111(4), 1999, pp. 589–592

RESPONSE

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Hubbard (1999) criticizes our paper *Migration of the Willow Flycatcher along the middle Rio Grande* (Yong and Finch 1997), where we reported aspects of stopover ecology of the species including timing, abundance, fat stores, stopover length, and habitat use. Hubbard questions our identification of subspecies of the Willow Flycatcher (*Empidonax traillii*) and the methods we used to identify them. He also attempts to evaluate the accuracy of our results of subspecies composition by comparing them with data from other researchers. We welcome and applaud this scrutiny in the hope that this interchange will stimulate greater interest, research, and capability to distinguish the phenotypic characteristics of subspecies of the Willow Flycatcher. Given that the southwestern race (*E. t. extimus*) of the Willow Flycatcher is federally listed as Endangered, reliable methods for identifying this subspecies need to be developed to more effectively conserve and recover its populations.

We are aware that the subspecific taxonomy of the Willow Flycatcher is inconsistent among taxonomists as are the techniques to identify subspecies. Consequently, reliable identification of subspecies is difficult, especially in field situations. We acknowledge that issues of taxonomic status, population distributions, and identification methods of subspecies of the Willow Flycatcher should be explored further. However, Hubbard's criticisms of our paper are generally based on erroneous information as well as incorrect assumptions about our methods, and they do not alter our conclusions about Willow Flycatcher stopover ecology at the species level.

Hubbard's first criticism focuses on the methods we used for identifying the subspecies. Rather than using an assemblage of subspecies skins as advocated by Hubbard to identify Willow Flycatcher subspecies in the field, we relied on descriptions and records of coloration and morphology published in the available literature by taxonomists. Contrary to what Hubbard speculates, we did not convert color descriptions into Smithe's (1975) color code values. We based our identification of back plumage color on the most recent research by Unitt (1987) and Browning (1993). Using Smithe's color codes to describe back plumage, Unitt (1987) writes: "In *brewsteri* the green is in the direction of olive green (color 48), in *adastus* in the direction of greenish olive (color 49), and in *extimus* and *traillii* in the direction of grayish olive (color 43). That is, *brewsteri* is a dark brownish olive, *adastus* a dark grayish green, and *extimus* and *traillii* a pale grayish green. . ." Browning (1993) suggested that Smithe's color system is problematical because the color swatches generally are not identical matches for actual colors. Hence, he used Munsell Color Charts (1990) to describe the crown and back contrast for his specimens. During our fieldwork, we consulted both Unitt's (1987) color codes for subspecies' back color and Browning's color contrast scores between crown and back. Although Hubbard suggests that live specimens have some disadvantages, we counter that the plumage coloration of live birds is more likely to be true to type than skin specimen plumage that may have faded. If our hypothesis that the coloration of fresh plumage differs from that of faded plumage is correct, then data collected from live specimens may be more reliable, or at least not less reliable, than results obtained from study skins. Birds occasionally called or sang in our study after being released. Information about song and call characteristics were also recorded when possible. Such data are available from living flycatchers but not from skins. Sedgewick's (pers. comm.) preliminary analyses of Willow Flycatcher song and call signatures collected

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in different regions suggest that *E. t. extimus* song structure can be distinguished from that of its northern conspecifics and we used this kind of data to aid identification also.

We did not rely solely on coloration for subspecies identification, contrary to Hubbard's second assumption. Unitt (1987) suggested that wing formula (relative length of primary feather length) can be used to assist subspecies identification. Of the 305 specimens that Unitt (1987) examined, wing formula distinguished 93% of the *E. t. extimus* and *E. t. traillii,* 88% of the *E. t. adastus* and *E. t. traillii,* and 89% of the *E. t. brewsteri* and *E. t. traillii.* Browning (1993) also applied wing formula to assess variation in subspecific characteristics, and his results also demonstrated that wing formula may be useful for distinguishing some subspecies although his sample size was smaller than Unitt's. Hubbard himself (1987) noted that *E. t. brewsteri* was smaller than other described forms. In the field, we relied partly on non-overlapping extreme wing measurements to assist in the identification of this subspecies. In addition, we measured and recorded more than 30 variables from each individual. Following Unitt (1987), we used wing formula to aid in identifying subspecies.

Thirdly, Hubbard (1999) comments that "even when characteristics of populations are better known, opinions may differ as regards their taxonomic treatment" because of limited sample sizes, interbreeding among populations, and differences in taxonomists' methods, views, and findings. Although we agree that taxonomists have been inconsistent in their treatment of subspecific taxonomy, we consider this to be an incentive for finding areas of common ground among researchers, rather than a justification for concluding that reliable identification of subspecies is impossible. Hubbard states that we should have strictly adhered to a single view of subspecies taxonomy. We followed a single view of subspecies treatment, but we did not credit this single view to a single researcher. We made it clear that we adopted the "four subspecies classification system of Hubbard (1987) and Unitt (1987)." We warned readers in our Methods section that: "Given morphological overlap and hybridization among subspecies, complete accuracy in identifying subspecies is not achievable." Although taxonomists disagree in their interpretations of within-species variation and subspecies recognition, there is unmistakable agreement about use of a four subspecies classification among recent research papers (Hubbard 1987, Unitt 1987, Browning 1993). Hubbard (1987) clearly advocates acceptance of the four subspecies classification in his report by stating that: "Given the degree of agreement among recent workers, I believe the most prudent course is to accept all of the above subspecies [i.e., *E. t. extimus, brewsteri,* and *adastus*] and *traillii* as valid—at least until more definitive studies are available." Although in his commentary Hubbard declares his own report to be a "cobbling job", its quality is deemed sound by other authorities. Indeed, it has been widely distributed and cited both unofficially and officially by the Endangered Species Programs of U.S. Fish and Wildlife Service regions, by state Game and Fish Departments, and by other agencies and ornithologists in the western United States, especially in the Southwest. Given Hubbard's background as a competent taxonomist in New Mexico and as an officer of the state endangered species branch, his paper is judged as an authoritative source on the species. For example, in the process used for listing the southwestern Willow Flycatcher as a federally endangered subspecies, Hubbard's paper was one of the most heavily cited reports by the U.S. Fish and Wildlife Service (1995).

Unitt (1987) also states that the four races of *E. traillii* are valid and may be distinguished from each other by "color, wing formula, or both". Browning (1993) further separated subspecies *E. t. traillii* into two populations: *E. t. campestris* of the Great Plains and Great Lakes regions, and *E. t. traillii* to the southeast of *E. t. campestris.* We recently became aware, that Unitt has conducted further research on the same specimens and may soon be updating his taxonomic treatment (P. Unitt, pers. com. through J. E. Cartron). These different authors describe subspecies distributions that are very similar although population boundaries are not exactly the same. U.S. Fish and Wildlife Service relied partly on these studies to conclude that listing the southwestern Willow Flycatcher as an endangered subspecies was appropriate.

Fourthly, Hubbard evaluates our results by

comparing our subspecies composition data with subspecies data from his own and other reports and sources. While such comparisons may be valid for the purpose of exploring potential sources of variation, the conclusions that Hubbard draws are incorrect because of spatial and temporal differences among studies. Species, subspecies, and population composition of migratory birds captured at specific stopover sites in fall or spring can dramatically differ from what is observed at the same location during the breeding season at the same location or from other locations during migration. For example, the overall species composition we detected indicated that the majority of individuals captured were not local breeders and many did not even breed in New Mexico (Finch and Yong 1999). While we used a standardized, systematic procedure to sample throughout the entire migration seasons of spring and fall, 1994 and 1995, other studies that Hubbard (1999) cites and compares to ours were not conducted during migration seasons and/or did not use standardized procedures. In addition, source studies cited by Hubbard are heterogeneous in relation to study goals, year of study, number of years, geographical location, sampling design, sampling season, and quality of data, leading to uncontrolled and unknown factors that invalidate comparisons with our data set. Our data are restricted to two sites during two years in the middle Rio Grande valley of New Mexico, and thus are only truly comparable to other data from the same vicinity, year, and sampling design. Given that different studies, especially earlier ones, used controversial criteria for classifying and counting their specimens, Hubbard's argument that our results are inaccurate because they are not completely consistent with other studies that, when compared, also yielded dissimilar results is circular. In our manuscript, we did not make such comparisons for at least two reasons: (1) our research focus was on the stopover biology of the species, not on the taxonomic status of the subspecies, and (2) other data sources were not homogeneous or similar enough to draw comparisons.

Our data and conclusions about the flycatcher's stopover ecology are not dependent on the validity or accuracy of its subspecies status or on the methods used to identify subspecies. Because *E. t. extimus* is endangered, U.S. Fish and Wildlife permits for collecting voucher specimens during migration are not issued in the Southwest, eliminating the possibility of having an alpha-taxonomist identify locally caught specimens to subspecies for the purpose of setting standards. Because most current research studies and conservation efforts pertaining to the Willow Flycatcher have focused on its breeding grounds, the importance of our research centers on when, where, and how migration stopover sites in riparian woodlands along the middle Rio Grande are used for resting and fat depositions by the species. Without understanding the migration strategy of the species and without justifying efforts to conserve the stopover habitat that the species uses, the Willow Flycatcher's fate in the Southwest will be jeopardized regardless of how perfect or imperfect our ability in identifying subspecies is.

Throughout ornithological history, subspecies classification and identification have traditionally been a "problematic" area, particularly within the genus *Empidonax*. Uncertainties about subspecies or even species status do not negate the value of our migration research or refute our results about Willow Flycatcher stopover ecology or intraspecific variation in migration patterns. We assert that increased knowledge of the stopover behavior and energetic condition of the Willow Flycatcher is important for understanding the biology of the species as a whole and that information about within-species variation is valuable in conserving the endangered southwestern subspecies.

Our paper and Hubbard's (1999) critique have opened up the opportunity to develop and expand discussion and evaluation of the different subspecies, the subspecies concept as a whole, and whether subspecies should be recognized for the Willow Flycatcher given the disagreement about their identification and the difficulty in identifying birds in hand. We invite and challenge others to contribute ideas and knowledge to this controversy in the hope that new or better techniques for identifying willow flycatcher subspecies may result. Such discussion or results would certify beyond a doubt the worthwhile contribution of our paper. Subjecting any paper to a critical commentary, however, automatically attracts the

notice of additional readers. We are pleased with the extra attention in the hope that further research, understanding, and conservation efforts will be directed toward the endangered southwestern Willow Flycatcher and its disappearing habitat.

ACKNOWLEDGMENT

We thank R. Banks for his review of the manuscript.

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